

Berlin, Nathaniel 1997

Dr. Nathaniel Berlin Oral History 1997 B

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National Cancer Institute Oral History Project

Interview with Nathaniel Berlin, M.D.

conducted on June 30, 1997, by Gretchen A. Case

at History Associates Incorporated in Rockville, Maryland

GC: The way I usually start out is just having you give me a little bit of background about your education and what you did before you came to the National Cancer Institute.

NB: All right. I'll go back—I should have sent you a CV but I didn't.

GC: That's okay. I read a lot about you. But if you could just tell me a little bit about how you ended up at the NCI.

NB: Well, I first of all went to high school in Miami Beach. I'd long had an interest in medicine. My father was a physician. Then I went to Western Reserve, now Case Western Reserve University, where I was a premed with a major in chemistry. I then went to medical school at what is now the State University of New York. At that time it was the Long Island College of Medicine. It was where my father went to medical school when it was the Long Island College Hospital. And then I interned at the Kings County Hospital in Brooklyn in 1945 and 1946; was a resident in pathology for one year at that same hospital. Then I went to Berkeley in June of 1947 as a graduate student in the Division of Medical Physics of the Physics Department. In the fall of that year, John Gofman, who was one of my teachers, said there were fellowships being made available by the National Institutes of Health. So in the fall of 1947, I applied to the National Cancer Institute for post-doctoral research fellow, which I was awarded in April of 1948. In '49, I received a Ph.D. degree from Berkeley. I stayed on the staff—I had a minor appointment on the faculty, a very junior appointment. In 1952, I decided it was time to move on—

GC: From Berkeley?

NB: I didn't want to leave Berkeley. But for scientific reasons. I went to England as a National Heart Institute Special Research Fellow. At the same time that I decided to move on scientifically, I thought about leaving Berkeley. In December of '52, about the same time that I was awarded the Heart Institute fellowship, I was interviewed at the National Cancer Institute by G. Burroughs Mider, whose name you might have heard.

GC: Oh, yes.

NB: And by Len Fenninger.

GC: That's not a name I've heard.

NB: Len was more or less organizing something which I eventually took over, which I'll tell you about.

GC: Okay.

NB: He left there, went to Rochester, and then to the AMA, so he disappears, but Mider stayed on. And Mider offered me a job, and I turned him down. I said, "I'm going to take the fellowship." He said, "Well, come on to work for us and we'll send you to England." But I liked the fellowship because they gave me freedom, in retrospect. And then what Bo said was, "Keep in touch with us." So I did—which was his opening of the door. While I was in England, I got drafted into the Navy, under the doctor draft era of the 1950s and 1970s, which we'll probably come back to during our discussion.

GC: Okay.

NB: If we don't, remind me.

GC: I will.

NB: Please. I went to England, but about nine months into the fellowship I got drafted and came home, went on active duty in the Navy in California, and then, for one reason or another, I came to Washington because the Navy had a problem with me. There were two competing forces in the Navy with respect to an assignment for me. There was a man named Howard Karsner, who had been professor of pathology at Western Reserve, who was the research advisor—I guess he was the senior civilian scientist advising the, technically known as the Bureau of Medicine and Surgery, who knew of me, but I'd never met him. There was a group in the Surgeon General's office who knew of me from my work at Berkeley. While I was at Berkeley, I held a Q Clearance from the Atomic Energy Commission, and I went over and interviewed at the Pentagon, and went to work for the Armed Forces Special Weapons. The Q Clearance was helpful. But I also went out to Bethesda, and I went in to see Bo Mider. I got into his office, and he chatted with me—you know, we'd corresponded, didn't know each other well—showed me some data—and I'll only demonstrate how naive I was or how simple the academic career system was in those days. And Bo said to me, "Nat, when are you going to come to work for us?" And I said, "The day I get out of the Navy." That was the offer of my job and my acceptance.

GC: Oh, really?

NB: And about a year later, Zubrod had come on as Chief of Medicine of the General Medicine Branch, and Clinical Director. And he met with me, he asked me to come up from the Pentagon, and I met with him, and he offered me the job as head of the Metabolism Service in the General Medicine Branch. That's how I got to NIH.

GC: And so you began working—were you working under Dr. Zubrod?

NB: Yes. The General Medicine Branch had four units in it. One was run by Dave Rall, it was Experimental Pharmacology. One was Dermatology, run by Gene Van Scott. One was the Leukemia Unit, run by Emil—you probably know that we all called him Tom Frei.

GC: Right. I actually talked to him, too.

NB: Yes. And one was mine, the Metabolism Service. That was in November '56. Around the end of '58, early '59, Bo Mider left the Cancer Institute and became what was essentially the Deputy Director of NIH. But that job—the Deputy Director for Intramural Research—has a variety of titles over the years. I think when Bo had it, it was Associate Director for Intramural Research. Then it became the Director of General Laboratories and Clinics. Then it went back to Deputy Director of Intramural Research, and I think that's what it's called today.

When he left, he was the Associate Director for Intramural Research for the Cancer Institute, which was the scientific—the formal title for the Scientific Director at that time. When Zubrod gave up the General Medicine Branch, I took it over.

GC: Right. And that was in '60?

NB: About '59.

GC: '59?

NB: About there.

GC: Okay. I've seen some confusion, like was it '59, '60, '61? With conflicting sources, it's hard to—

NB: In that era—well, I can piece it. I don't have the documentation. I think it was in '59 that I became Chief of the General Medicine Branch, which made me, in effect, the Chief of Medicine because those other three units reported to me.

GC: Yes, okay.

NB: Then when Bo Mider became Scientific Director, Zubrod succeeded him as the Scientific Director, and I succeeded Zubrod as Clinical Director, and we dissolved the General Medicine Branch. So I kept my own unit, the Metabolism Service; Dave Rall became head of a branch; Gene Van Scott became head of Dermatology; and Frei had—I think we called it the Medicine Branch then.

GC: I think that's right.

NB: But as the Clinical Director, Surgery reported to me, as did Endocrinology, and Radiation. The Clinical Director's position was structured in the Office of the Director, formally, and they created some minor sort of title for me, under Zubrod, as Scientific Director. I was under him as an Associate Scientific Director. Jack Masur, whose name you probably haven't heard yet—

GC: I have.

NB: —was the Director of the Clinical Center, at least shortly after I got there. Jack used to call the Clinical Directors the Physicians in Chief, which was a more familiar title in medical schools and in hospitals. One of the Clinical Center units, the Pathology Branch, was also an NCI branch that reported to me.

GC: Okay.

NB: One of the major responsibilities of the Clinical Director was the quality of patient care . . . aside from the scientific oversight and the administrative management of all the clinical branches.

GC: What did that mean in terms of what you had to do? Did you make rounds?

NB: Oh, sure.

GC: Did you talk to patients' families? What kind of—

NB: I made rounds. First of all, I kept my job in the Metabolism Service because that was my scientific personal home, and it was a very successful unit.

GC: Yes, that's what I've heard, that it was very successful. Dr. Zubrod said it was probably—he said it became the outstanding branch in the whole NIH.

NB: No kidding!

GC: Yes, that's what he told me.

NB: He used to tell me that. I didn't realize that. But I did make rounds on all the services, every one of them, periodically. My job when I made rounds was to find out what we were doing, how we were doing it, and what the problems were in the delivery of health care. And I was the official interface between the Institute and the nursing service, as the Clinical Director; the official interface between the Institute and the Clinical Center, particularly all the Clinical Center Departments. So I did make rounds, I talked to patients, I admitted my own patients, I talked to families, I make all sorts of interesting decisions at times. Amongst one of the examples, we—all the bedrooms, the patient rooms, were two-bed. And one day one of the nurses came to me and said, "Dr. Berlin, this white woman doesn't want to be in a room with a colored woman." And I said, "She may leave." Her medical care—there were no bills, no physicians' bills, no hospital bills; they came entirely free. If she didn't want to stay, let her go.

GC: Wow. What did she do? Do you know?

NB: I don't know. I just said, "She will not be moved to satisfy a basic prejudice." I'm no more prejudiced—I'm probably less prejudiced—no, no more prejudiced today than I was then. But I wasn't going to let any white woman come in and tell me that she wouldn't be in a room with a black woman.

GC: No. Wow.

NB: Those are amongst the things. We conflicted a little bit with the hematology people in the Clinical Center. A man named Brecher got very angry with me because he didn't agree with what we were doing clinically in the acute leukemics. He thought we were requesting too many laboratory services. And he had a bad habit, by my criteria. He would call up the younger members of the staff and berate them. And finally that went on too long, and I went to either George Williams or the Director of the Clinical Center, Masur, and I said, "This has to stop."

GC: And did it stop?

NB: Oh, sure!

GC: So you really defended your staff.

NB: When they were—I wasn't going to tolerate it. They were there to service and that was it. Brecher later went to one of my friends and berated me in terms I will not repeat.

GC: Did he ever speak to you in person, or just to your friend?

NB: Oh, yes, he always spoke to me. He spoke to me in person. He was angry, that's all. I still see him occasionally and we greet each other cordially. George Williams was not happy. He was Chief of Clinical Pathology. So that's how I functioned as the Clinical Director. All new appointments had to cross my desk. All appointments to the medical staff had to be approved by me. Zubrod and I jointly appointed Al Ketcham as Chief of Surgery. Zubrod was the Scientific Director then. When I became the Clinical Director—and whether you include it or not—there were two Department Heads: Roy Hertz, who was Chief of Endocrinology, and Bob Smith, who was Chief of Surgery. Bob came to me and said, "Now that you've got the job I wanted, I'm leaving." Roy Hertz came to me and said, "I didn't want the job," which wasn't exactly factual. But I got it anyway. So when Bob Smith left, we appointed Al Ketcham. And when Roy Hertz left, we appointed Mort Lipsett. Later on, most of the appointments were mine.

Zubrod established a meeting once a month with the Chief of the Autopsy Service. And he and I went over every autopsy. For a long time—in my initial days as the Clinical Director—and I don't know how long I continued it—I read every discharge summary.

GC: You did a lot of paperwork, then.

NB: I did a lot of paperwork, and I was very visible.

GC: Were you?

NB: Yes. You know, I also chaired and organized our Friday morning meetings, which were the grand rounds. I organized the combined—there was a combined clinical staff conference, which I organized—we'd try to present broad programs. For a decade, I sat on the Medical Board, and chaired it once. In the mid-'60s, I chaired what was known as an ad hoc committee. It included Don Fredrickson, Jack Masur, I think Maitland Baldwin, who was Chief of Neurosurgery. This was the committee that revised the rules for the governance of clinical research. It took us two years to do it. And then we issued a very brief paper, and it is the process that's become standard throughout the academic community. It's led to the Institutional Review Boards.

GC: It's amazing that something that you did at NIH could have such a broad effect on the medical community.

NB: Well, what happened then is Roger McDonnell was Bo Mider's assistant, and either Roger or Bo called me up, I went over it, and pretty much orchestrated its introduction into the universities through the grants mechanism. Then they created an Office for the Protection of Research Subjects, I think—don't hold me to all these titles. By the way, if you get the phone books—have you ever looked at the NIH phone books?

GC: I have, yes.

NB: They're a valuable resource.

GC: Down in Victoria Harden's office?

NB: Yes.

GC: Yes.

NB: Which was originally run by a man named Chalkley, who came up from the Department downtown. I didn't like the way he ran his office anyway. In the middle, towards the end, I sat on the Search Committee for the Directorship of the Heart Institute, for the Directorship of the Clinical Center, I chaired the one to find the Chief of Anesthesiology, which was not successful. We contracted with Georgetown to provide the anesthesia. We just couldn't match salaries in the private sector. I got to know a number of anesthesiologists around the country while we were doing that. So those are some of the things that I played some role in, in terms of the Clinical Directorship. But essentially, it was—one of the more interesting things that I did, we had a matching program—have they told you about that?

GC: A matching program? Tell me a little bit more.

NB: This is how we recruited junior medical staff. In my time, they all came under the draft. In the spring of the year, we would interview fourth-year medical students for appointment two years later. We called this the Matching Program. The title was derived from a national matching program for internships. So I interviewed those people.

GC: The medical students?

NB: Yes. I interviewed them—I interviewed some who were to work with me personally. But I didn't interview all the others. What I did when I became the Clinical Director was to decentralize the matching so that the interviews were conducted, the surgeons' interviewed for the surgical positions, the medicine people interviewed for theirs, the dermatology for theirs, but I reviewed all their nominations that crossed my desk.

GC: So things went through you first, and then off to the—

NB: Matching.

GC: Right.

NB: We had a regular Matching Office.

GC: Okay. And while you were doing this, you were still running the Metabolism Service, right?

NB: Yes, that was easy.

GC: That was easy?

NB: Oh, sure.

GC: Will you tell me more about how you built up the Metabolism Service?

NB: Oh, sure.

GC: Why was it easy?

NB: It was small, we were compatible, collegial largely. When I came to the NCI, as I told you, Zubrod offered me the job. There were four people there who had coalesced in one fashion or another. These were Donald Watkin, who didn't stay very long. Don became interested in nutrition, traveled a lot, and was a major user of the Metabolism Service's eleven beds on 3B. At one end of the corridor was a kitchen, and at the other end of the corridor was a laboratory, basically a metabolic balance laboratory. The patients were put on a constant diet that was made up by the dietician and frozen. And at the other end where urine and feces came, that's where they're analyzed for their content. So it was self-contained.

GC: Yes, I guess so! One end to the other, right?

NB: And the nurses were very good because the patients could only eat what they were given, and they could select a standard diet when they came in, and that's what they got every day . . . rigidly and rigorously. And if the nurses suspected there was a candy bar around, they'd go search.

GC: Oh, really?

NB: Yes. But we only had eleven beds, and there were five of us. Don Watkin ran the Metabolic Balance Lab. I didn't know much about it and wasn't that much interested. I used it later in my career. Jesse Steinfeld was there. Jesse I'd known from my days in California, when he was a young Public Health Service officer at the Laguna Honda Home, which was a chronic disease home in San Francisco. Have you heard about it?

GC: Is this where Michael Shimkin set up shop?

NB: Yes. He was one of Mike Shimkin's people. There was a blood club, and I met Jesse there. And then he turned up on my service at the NCI. He was very cordial to me. Jesse was interested in albumin metabolism. He also did a lot of the work with thyroid uptake because of the skill he had. The Laguna Honda Home under Mike Shimkin—and I'm diverting, but you'll get it—was essentially created to be a staging ground for the Clinical Service.

GC: Oh, really?

NB: Yes. The Clinical Center opened in '53, either '53 or '54, I forget which.

GC: '53.

NB: Incidentally, the first patient was admitted by Roy Hertz from the Cancer Institute.

GC: I've heard that.

NB: God, you've heard a lot. I can't tell you very much new.

GC: Sure you can!

NB: At the Laguna Honda Home, Mike Shimkin ran that as a cancer clinical research unit. There was Howard Bierman in it, Keith Kelly, and Laurie White. I see Laurie White occasionally. He later became President of the California Medical Association. Mike was hoping, I think anticipating, that he would come back and be the Chief Physician of the Cancer Institute, but this did not evolve. Mike was unhappy about it. They gave him a nice job as Associate Director of Field Studies. He and I were friendly—as a matter of fact, Mike and I did a piece of work together when I was at Berkeley and he was in Laguna Honda.

GC: Oh, really?

NB: A paper published in the *Journal of the National Cancer Institute*, one of the first papers I published. And Don Tschudy had some down from Columbia. He was interested in a non-cancer sort of disease, porphyrias. And John Fahey was there. John had come down, went to the Harvard Medical School, I think he was at Columbia or Presbyterian Hospital, but I forget which.

John's initial interest was metabolism of the amino acids. Then he shifted to immunology, and eventually went to UCLA as Chairman of the Department of Microbiology and Immunology.

Jesse Steinfeld did not stay very long. We were very good friends, but I never found out until many, many years later why he left.

GC: Oh, really?

NB: At least what my wife says his wife told her. At Berkeley I learned the fundamental lesson that we don't like to know, we don't like to acknowledge in the academic community. But have you heard the phrase, "Publish or perish"?

GC: Yes.

NB: And what I did is I kept monitoring—these were the four of us, let me go back. And if you go back and read what Mider wrote in a couple of reviews before I came on, the Metabolism Service basically had its godfather with Mider, because he thought we should study the anemia of cancer, weight loss, nitrogen metabolism, things like that. And I think those were the things that attracted me—no, that caused him to get interested in me. Because the Metabolism Service was set up without any specific responsibility.

GC: Oh, really?

NB: We did a lot of work that wasn't cancer. It was essentially a human physiology group. But Jesse left. He eventually ended up as President of the Medical College of Georgia, before that the Dean, I think, at the Medical College of Virginia, for a brief time was Director of the Cancer Center at the Mayo Clinic, before that he was the Surgeon General of the Public Health Service. And his name has—that must have crossed you.

GC: Yes. He's been everywhere. I saw him—you wrote about him, too, in one of the articles.

NB: And shortly after I got to the NCI, there were three newly arrived physicians. In December of '56, Gordon Zubrod asked me if I'd interview Dave Nathan, and I did. Dave and I are good friends. It wasn't until about three or four years ago he told me that when he left my office after that interview, he was angry . . . very angry.

GC: Why was that?

NB: Because I wouldn't let him do what he wanted to do. He tells the story that I was sitting there—I used to have a chair that rocked back, and I'd sit in it with both my—sit in it like this.

GC: With your legs crossed up?

NB: And he said, "like Buddha." And he was there, and he said—we talked about research. And he said he wanted to study some aspects of liver disease. I knew nothing about liver disease. My experience had all been in red cell diseases, in particular. And he says, and he wrote it in his book—and I don't believe him still to this day, but there are elements of truth to it—I said, "Dr. Nathan, if I were a commissioned officer"—and I'd just gotten out of the Navy by the way—"how many stripes would I have on my sleeve?" And he said four. Do you understand how Naval officers wear their rank?

GC: I think so. Would four stripes have been a Captain?

NB: Yes. And, "Dr. Nathan, how many do you have?" He said, "Two." So finally we agreed, during the regular working hours, he would do his research with me, along some preconceived notions. And in his spare time—and he did do some work with liver disease, not much—

GC: In his spare time?

NB: Yes. And he writes in his book and he tells these people that he was ordered into hematology. He eventually ended up as President for the American Society of Hematology, and now is President of Dana Farber. He was the Chief of Hematology at the Children's Hospital.

GC: So it was okay in the end.

NB: Yes. He was the first one. Then Zubrod asked me or Mider asked me if I'd interview two fellows, Waldmann and Weissman. And I don't remember all the details, but they came into the Institute—all three of them came in July of '56. I got there in December. And there was some notion that they were going to work in Jesse Greenstein's biochemistry lab. That didn't work out. So I interviewed them, and they came into the lab. So there were the four of us. And Nathan and I did some work together. Waldmann and Weissman had gone to the Harvard Medical School together. And in those days in medical schools you were lined up alphabetically. So Waldmann was next to Weissman in anatomy or wherever else they worked, and they were a pair. So they came to me as a pair, and we did some work together.

GC: And did they continue to work well together as a team?

NB: Yes. Eventually the three of us went our separate ways. Weissman went off to finish his training in medicine at the Illinois Educational Research Hospital, and then to a man named Davidson at Glasgow, and then came back to the Metabolism Service. Waldmann and I did a few things together. But I learned some lessons at Berkeley. One is I was not going to dominate people. Now when it was about a year before Waldmann would have completed his obligatory medical service, I went to Mider and said I wanted to keep him. Mider told me no. But he said, "Nat, I'll arrange for an American Heart Association Fellowship for Waldmann with you as his preceptor." And so Waldmann stayed on for a year as a fellow to the American Heart Association, then was appointed to the staff, and he's never left the Metabolism Service.

Towards the end of the '60s, I began trying to get him [Waldmann] to take that job away from me, and he wouldn't. But he accepted a position as the Deputy Branch Chief in around '71. Around '71 or '72, he did take it away. He didn't take it; I gave it. I didn't need it. I kept my lab there for a while, then I gave up my lab, when I moved from the Clinical Center to Building 31. Then Lee [Leon] Rosenberg came on pretty much as a successor to Don Watkin. We sent him away to Yale for a year, and he came back, stayed about five or six years, then went back to Yale, where he eventually became the Dean. Then he left Yale to become the head of Bristol-Myers-Squibbs' research arm—I think they called it the Pharmaceutical Manufacturers Institute in Princeton. And that's the early days of the Metabolism Service. We were successful in recruiting young physicians as clinical associates. What I did is I established a pattern that each one of us would interview and then select on his own. So there was the further decentralization.

GC: Even within the Service, you mean?

NB: Yes.

GC: You each selected.

NB: Essentially, I tried to be an egalitarian—whether that's modest or immodest, I don't know—on the research side we each functioned independently. On the clinical side, we admitted our own patients. But we made rounds every Friday as a group, the five juniors, we had five seniors, with our head nurse and our dietician. And when they didn't agree what I was doing or suggesting clinically, they told me so.

GC: Oh, really?

NB: Yes. They weren't afraid of me.

GC: Oh, okay. So there was real open communication.

NB: Between me and the juniors; certainly between me and the seniors. I think so. Now, they may tell you, "Nat, you're deluding yourself."

NB: But they'd do it.

GC: Do you think this is part of why the Metabolism Service was so successful?

NB: Yes. Then on Mondays, we met for research at lunch, we'd have a luncheon seminar.

Why was it successful? Because we were collegial, for one thing. John Fahey was the least collegial, and Zubrod thought we ought to create a laboratory of immunology for him, which we did, and then it became even more collegial. And I think it was successful because I established a policy that each one of us would have a laboratory space of 450 square feet, and you'd have to carve a little desk out of it. You'd get one clinical associate for two years. If that man wanted to stay on for a third year, we did it, and often they did. And they would get two technicians and access to the beds. And if you wanted any more, you had to find it someplace else.

GC: Oh, really?

NB: Yes. And I had no more than anybody else.

GC: So you were on the same level as your seniors or your juniors in terms of resources.

NB: Yes. The juniors came and went, but the five seniors—I had no more access to the beds, I had no more laboratory space, and no more technicians.

GC: That does sound very egalitarian.

NB: Well, as I said, I learned my lessons at Berkeley. There were things that I was clearly not going to do, and one is to set up that group in my own image. I could have done it.

GC: And that's something you experienced at Berkeley from the other end?

NB: Yes. Because in Berkeley some of my friends set up big units, and I didn't like it. I don't like central suns with a lot of planets around them in the research world. But that was me. Now Waldmann has changed that a little bit. Not a lot. You'd have to talk to Tom. I'm sure he's not changed it a lot. He has more space, some of the others have more space, they may have more technicians. But when the doctor draft went off around '71 or '72, our ability to recruit young physicians disappeared. And Tom's had to accommodate to a new way of staffing with juniors. And, of course, he's developed as a very major figure in the immunology world. Mike Blaese was on the Service. He came as a clinical associate with Tom. I forget how Jay Berzofsky came, of the present staff. I just don't remember. Peter Nissley came on when they wanted to make room for some endocrinology. And the first group that was there—amongst those who had been recruited in '55 as young physicians was a young man, Daniel Nathans. He later became the Acting President of Hopkins, but more importantly, got the Nobel Prize.

GC: That's right. And was the Nobel Prize for work that he—

NB: No, work that he did when he left us. But he started there.

GC: That must have been an amazing experience for you to see someone who had been in your lab go all the way to the Nobel Prize.

NB: Well, I think I wrote that we've had a—that group there—of the clinical associates and the seniors there's been a very successful group, some in different ways. Steinfeld, for example, had known in southern California Roger Egeberg who became an Assistant Secretary of HEW, and it was he that brought Jesse back from California to a position which eventually led to the position of Surgeon General. Jesse left many years ago—I started to tell you "publish or perish"—

GC: Right. Can I stop you for a minute? I'm going to flip the tape.

GC: Okay, "publish or perish."

NB: Yes. Jesse's wife and my wife were very friendly. They would talk to each other. And some years later Barbara says that Jen Steinfeld said Jesse left because I was pushing him to publish. And I never thought I was—you know, you have or to remember, or you have to recognize what I thought was a little nudge—sometimes people would say that they got batted over the head.

Watkin left because he got too interested in research in nutrition, internationally. And he wasn't compatible with what we were trying to do. He also didn't publish—he used the metabolic balance laboratory extensively, but not a lot of that work never saw the light of day.

GC: Oh, really?

NB: Under me, men were recruited who I thought were good scientists, were interested in working together. Waldmann's tended to shift the interest in part to the immunology world where he's a very major leader.

GC: You said it took a long time to convince Dr. Waldmann to take over from you.

NB: Yes.

GC: Why was that?

NB: You'd have to ask Tom, but I suspect he didn't want the responsibilities.

GC: Oh, really?

NB: He wanted to be a scientist. And he didn't want to move up the hierarchical ladder. I moved up the hierarchical ladder very fast.

GC: Did you?

NB: When I became Chief of Medicine, I was 39. I was the youngest Department Head in the Cancer Institute.

GC: Wow. That's amazing.

NB: When I became Clinical Director, I was about 40. I was the youngest of the Clinical Directors. You might say I was on a fast track for awhile; I never realized it.

GC: So it wasn't something you consciously did? It just was the way your—

NB: I never asked for anything. And there are days when I say to people, "If I had it to do over again, I might, with hindsight, have stayed as Chief of the Metabolism Service."

GC: Why is that?

NB: To me it was very rewarding to create new data and see it, to take a problem and work through it. And when I say "to create new data," I envision myself as a scientist. Others might tell you I was a good administrator.

GC: But you saw yourself primarily as a scientist?

NB: I've had the recognitions as a scientist that I need. Zubrod organized in the mid-'60s a reorganization of the Cancer Institute, and we ended up with three scientific directors: one for virology, one for chemotherapy, and the rest which is a mix. And the intent of that was to give Zubrod the resources he needed for drug development and to give Rauscher the resources he needed for the viral development, which were major efforts. The Cancer Institute scientifically, while it may never have articulated it, made two major decisions as to the thrust of cancer research. Did I send you the papers I wrote in the *Conquest of Cancer*?

GC: Yes.

NB: They made two major decisions—or a major decision. On the one hand, it would do the work to develop drugs to cure cancer. That's the metastatic part of cancer, by drugs. The major problem clinically in cancer, then and today, is the treatment of disseminated—do you know what I mean by disseminated and metastatic?

GC: Right, that it's spread to other parts of the body.

NB: Yes. That's the problem today, that was one decision. And there was criticism of the way the drug development was going; they turned to Zubrod, who is a pharmacologist, and he was allowed to pick from within the Institute the resources that he needed. The other side, we were going to prevent cancer through viruses, through some—the virus was going to be a tool for the prevention of cancer. Because you have to understand that at that time there are any number of viruses that you could see under an electron microscope, which you could put into a mouse or a rat and they'd get leukemia.

GC: Amazing.

NB: And so the basic philosophy was, if there are viruses that cause leukemia in an experimental animal, why is man exempt?

GC: Right.

NB: And that turned out to be—it was excellent reasoning at the time; biologically it turned out to be that the tools the virologist developed became many of the tools of the molecular biologist. So much of what molecular biology came to be came from some of the work in the viral oncology world. They now are beginning to come back to understanding some of the roles of viruses. And Al Rabson pointed out to me in the late '70s—and I don't know whether he's really correct or incorrect—that the viral immunology may be akin to that of poliomyelitis. A lot of people get infected with the poliomyelitis virus; few develop the paralysis. So the Epstein-Barr virus is implicated in nasopharyngeal carcinoma. There's something—Gallo has something around the AIDS virus and Kaposi's. There's something there, and I just don't know the details. And then the molecular biologists or the molecular geneticists, whatever you want to call them, with Huebner and a few others, coined the term oncogene, which are viruses that got in the man in ancient days and persisted, and somehow or another they get activated. And all of that is more in the modern biology of cancer, which has become rather complex. And then there was left behind General Labs and Clinics, which had no specific missions, neither Rauscher wanted, nor did Zubrod want. Then I went away for a summer. I'd acted as the Scientific Director for General Labs and Clinics while Zubrod was detached. I went away to Berkeley for the summer.

GC: And which summer was this?

NB: About '65, '64 or '65, somewhere along in there. John Lawrence, who directed the Donner Laboratory at Berkeley, offered me a Regents Professorship, a visiting professorship, which I would have loved to have taken because the one unfulfilled ambition in life was to go back to Berkeley.

GC: Really?

NB: I met my wife there, I had friends there. It is a great university.

GC: It's beautiful out there, too.

NB: And I went to see Bo Mider. Now, I'm one of the few people that Bo has had to his house fairly frequently, that I know of. Bo was honest as the day is long, and I said, "Dr. Mider, this offer, can I accept it?" He said, "Yes, but you may not be the Clinical Director when you come back."

GC: That's a big decision.

NB: And I always thought—I never asked John Lawrence, because one of the reasons I left Berkeley was because John Lawrence was making me promises for my academic future which he wasn't delivering on. And I often thought in the mid-'60s, this was his way for bringing me out for a year and seeing whether I would succeed again in the Berkeley atmosphere. Now, I'll be honest with you—if the shoes had been reversed when I went to Bo and asked for a year off, I would have told Bo, "No."

GC: Really?

NB: Because the Clinical Directorship was an ongoing position, someone had to do the work. Six months maybe, maybe longer, would be reasonable. But an academic year, a full year, no. So that is one element. When I came back from Berkeley, I expected to be the Scientific Director. I found out that I wasn't. Gene Van Scott had the job for about a year. I was content as the Clinical Director because I reported to the Director of the Institute and I had the independence I needed, I had the Metabolism Service. Gene didn't stay very long. Ken Endicott thought about it, but I think he wanted Jesse to do it—Steinfeld—Jesse refused. I just sat and bided my time. And one day Ken Endicott called me in and said, "I'm going to appoint you." Many years later I found out that while I was at Berkeley, he'd polled the Basic Science chiefs, who voted against me—they thought I was too clinical—but eventually turned to me. And then I was very fortunate. A number of things happened. I was able to bring Ira Pastan in as Chief of Molecular Biology, courtesy of Ed Rall. Has that name appeared yet?

GC: Ed Rall? Yes. I don't know a lot about him, yet.

NB: Well, there are two Ralls, Ed and Dave.

GC: Right, right.

NB: One was in the Cancer Institute; one is in the old Arthritis Institute.

GC: Right.

NB: Ed came to me and said, "Nat,"—we were good friends. We came together originally in January of 1957 when we both found ourselves on the Institute's Radiation Committee. The Radiation Committee was the group that looked at both people and projects to use radioactive isotopes, so that the NIH could buy them from the Atomic Energy Commission under a general approval. And we became friendly. And he comes to me one day and says, "Nat, we've got to do something for Ira." At that time, Herb Sober, who was Chief of the Laboratory for Biochemistry, died—and there was no one in the biochemistry lab that I wanted to make the Chief. I thought it was a mixture of people who were good, but the best had left. And what remained were all right, but not great. And I had turned to Ed Kuff, who became the Acting Chief, and I told Ed that I wanted him to come in with a plan splitting the lab into two parts; put all the good ones together, and put all the ordinary ones together, and we'll gradually phase out the ordinary. I also felt the lab was too big for me to manage directly, by one man, and I didn't like big labs. I'll tell you a story about—put Jesse Greenstein's name, and we'll come back to it.

GC: Okay.

NB: Because that's one of the little anecdotes in my history.

GC: Okay.

NB: Because I'm trying to remember all the details. Ed did it for awhile, ran the lab for awhile, but he never came in with a plan for its dissolution. And there was a man named Bob Goldberger, also in Ed Rall's unit. Somehow or another, Bob came to my attention, and to make a long story short, we offered him the Lab of Biochemistry as it existed. So I didn't break it up. One of the first things Bob did was to bring Maxine Singer in, who would later become one of the more very distinguished women scientists, the President of the Carnegie Foundation.

GC: And she's still at the NCI, isn't she?

NB: Yes, she's still in the lab. I see Maxine occasionally.

GC: Do you?

NB: She was very angry with me when I left, because I recruited her about nine months before I left—there were some people and there were a couple of wives, and a couple of the men were literally fearful when I left. Off the record, don't put—Waldmann was afraid. Mones Berman was not happy. Bob Goldberger brought in Maxine Singer, and then he brought in Claude Klee all from the Arthritis Institute, and that really remade the Biochemistry Lab.

I sort of slowed down the Laboratory of Biology. But there were two women there, Katherine Sanford and Virginia Evans, who were major figures in the development of the technology of tissue culture, which they didn't use in the biological sense as much as other people. But everybody who uses the tissue culture techniques today are indebted to them. And I finally got them promoted to the super grades [higher pay grades]. When Jay White left the Laboratory of Physiology, I became Acting Chief for the Laboratory of Physiology. In retrospect, they should have known what the signal was, because that meant its end. It was not meeting our needs, although Scotty Pratt was there, who was very good, and eventually became Director of the Division of Computer Resources and Technology. And there were two superb radiobiologists, Rodney Weathers and Mort Elkind, but they eventually left, too. So the Lab gradually disappeared. And, as a matter of fact, it was in some respects—a lot of the people are gone, so I can talk—people who couldn't get along with others or their Lab Chief often found themselves in the Laboratory of Physiology.

GC: Oh, really?

NB: Yes.

GC: And then they were eventually phased out, I take it.

NB: Well, they gradually—you know, it diminished and diminished. One of the most difficult things I had to do in my life was to deal with one of my colleagues from my Berkeley days; he was an absolutely superb biochemist, Jim . . . came to the Cancer Institute before I did, was in biochemistry and then physiology, I knew him well at Berkeley, he was a very popular young man—we were all in our late twenties—his girlfriend was the best-looking girl in the lab by far.

GC: Oh, really?

NB: You know, you remember some strange things. He never married her; he should have. She later became a friend of mine when she moved here. One day I walked into his lab, it was dark, and you could write your name in the dust on the lab [counter] top, so I found him a job elsewhere in the Cancer Institute. Jim had been to my house, we were friends, there were days when we passed in the corridor, and I couldn't look Jim in the eye. I guess if you take responsibility that you have to take, as I had—and don't hold me to be immodest. Yes, I helped some people and I didn't help others. But Jim never lost his salary. And Jim was a man who was meticulous in the lab, did meticulous work, but didn't finish it. And that was again, the publish or perish. Bo Mider taught me a lesson. He said, "Start a project, and do it, write it up. Whether you publish it or not is almost less important than writing it up, but finish it."

GC: Bring it to that end.

NB: Bring something to a conclusion.

GC: And that's what this person, Jim, wasn't doing. Is that why you're saying his lab was dark, that he just wasn't putting in the hours?

NB: He wasn't working. I put no constraints on his ability, to have resources at least, laboratory equipment and supplies, and maybe even a technician. I also created a laboratory of pathophysiology for Pietro Gullino. And one time I put together a plan for a laboratory of theoretical biology. I was going to have three units in it: computational biology, DNA metabolism, and the third, mathematical biology. And I tried to recruit Sherm Weissman. Sherm was willing to accept. My good friend, Bob Berliner, when the salary ceiling was \$33,000, wouldn't let me pay him more than \$32,000, so Sherm didn't accept. He would have been great. But that laboratory became the Laboratory of Mathematical Biology with Mones Berman, who began in the Arthritis Institute, and he needed some more resources. That's the laboratory that Klausner came into, his initial appointment was in the Cancer Institute.

GC: Oh, really? I didn't realize that.

NB: And then one day Bo Mider said, "Nat, you've made more changes than any previous Scientific Director."

GC: Really?

NB: My last recruitment was Steve Rosenberg.

GC: Wow. And he's done very well, too.

NB: And if you get out his book, you can see how I did it. And I'll tell you the story.

GC: Okay.

NB: If you're interested.

GC: Yes.

NB: After Al Ketcham left—well, indicated he was going to leave, he hadn't left yet—all the Clinical Chiefs came to me and said, "Nat, would you be willing to appoint a Search Committee?" We'd never had a Search Committee in the Cancer Institute before. That was the responsibility of the Scientific Directors, and I wasn't going to give it up. But I didn't know how to tell them no. So we're in a room, maybe a little bit larger than this—"Who appointed you, Tom?" "You did, Nat." "Who appointed you, Marv?" "You did, Nat." "Who appointed you, Ralph?" "You did, Nat." When we finished, everybody had the same answer, and somebody said, spontaneously, "Nat, thanks for seeing us."

GC: And that was it?

NB: That was it. I knew—Bill Terry told me that Steve Rosenberg was available, well, could be available or could be approached. I got together the surgeons who had sat on our external advisory board—this was Frannie Moore at that time, who was Chief of Surgery at the Brigham in Boston; Bert Dunphy, who was in the process of moving from, I think at that time, from Oregon down to California; Jack Cole, who was Chairman at Yale; and Paul Adkins, who was, I think at that time, the Chief at GW. We discussed the position, and they all gave me advice, "Get a young man, who's well trained in surgery, who's interested in immunology, and can move Immunology." Under Bob Smith, who was the first Chief of Surgery, it became a unit that became very skilled in what was called radical surgery. And I'd come to the conclusion—this is one of the decisions you can make as a Scientific Director—I came to the conclusion that that wasn't going to continue because I didn't see the research potential. And one of the things a Scientific Director can do is guide the research through the appointment of Department Heads. You can't guide the research by telling somebody, "This is a project you ought to work on." At that level, at least. And then I met with the surgery branch, the junior surgeons, and the senior staff of the Surgical Branch, they told me I talked to the wrong people. So they gave me a set of names, and I got them in: Ben Rush, Walt Lawrence, I forget one or two others. And they gave me exactly the same set of recommendations. They didn't differ at all. So I became very friendly with Walt Lawrence, who later became a major figure in the cancer world, the President of the American Cancer Society.

And then I went up to Boston and I talked to Frannie Moore about Steve. Frannie wanted me to take somebody else. I wanted Steve. Steve took a substantial cut in pay personally, but what we were able to offer him was twenty-six beds, nine surgeons on the staff, he'd have eight junior surgeons and unlimited resources. So he took that, and he came to the Cancer Institute. But if you get out his book, he writes in this chapter in a nice way how he wanted to come to me, I was afraid we wouldn't come to each other, we should have said, "Steve, I want you, are you going to come?" Steve should have said, "Nat,"—it would have made my life simpler—Steve would have said, "Nat, I want to come to work," I said, "Let's do it." In his book he says he went in to tell Frannie Moore—whom I know; Frannie was a good friend of mine—he told Frannie that he was leaving to come to the Cancer Institute. He said Frannie didn't answer, turned his back on him, looked out of the window, and that was it. Uncharacteristic of Frannie Moore. Well, of course, what Steve did, he brought Alan Baker in, and then another man, and literally took away much of Frannie Moore's capacity to do research.

Do you want to hear about the Breast Cancer Task Force?

GC: I do. That's something I wanted to talk to you about.

NB: Did anybody ever show you the report written in 1974, the report from the Breast Cancer Task Force to the Profession?

GC: I don't think so.

NB: Okay.

GC: Is that something I should look for?

NB: Zubrod conceived the notion of task forces, under the original concept of the task force, of bringing a small group of people together for a specific objective, and the first he created was the Acute Leukemia Task Force, which he ran very successfully. And I think he had another one.

In the '50s and '60s . . . one of the major ways of treating breast cancer was hormonal manipulation. You either gave some estrogens or you gave some androgens. And I once ran a meeting on that subject—I forget what the criteria were—for which women got a female-like hormone and which women got a male-like hormone. And within the Cancer Chemotherapy National Service Center, there was a group under Erwin Vollmer who had the responsibility for developing new steroids. Basically they developed a number of new steroids. They developed the notion that they would test these steroids in twenty women. The biostatistician said twenty women was enough for an initial test to see whether the drug would do anything; not how good it was, but whether it would do something. And this went on and on. And Ken Endicott later—Carl Baker had become the Director then—maybe in the transition period between Ken and Carl—they came to the conclusion that that had to change. And I guess I was elected the vehicle of change.

So I became the Chairman of the Breast Cancer Task Force. And we met, the endocrinologists and a few others, including Bernie Fisher. Has the Bernie Fisher name come to your attention?

GC: Yes.

NB: If you'll remind me, I'll tell you a lot about Bernie.

GC: Okay.

NB: We met in the room. In those days my stomach was bad. Bernie is about six-foot-two or -three, maybe 210 to 220 pounds, and he says—describes to people—this little man [Berlin] comes into the room, plunks some Maalox before him and some milk, and says, "Things are going to change."

NB: And the meeting didn't last very long. And later they went up—either that day or later—they went to Carl Baker and asked Carl to replace me as Chairman.

GC: Really?

NB: Because the endocrinologists who were—I don't know what they thought of me—but I said it was going to change, there was no question, and it did. And Carl said no. And Carl asked me to develop a plan, which we did do. We created five committees: a Biology Committee, an Epidemiology, a Treatment, a Diagnosis, and I forget what the other was. Carl increased the budget very substantially, and we appointed five contract review groups. Vollmer did the staff work and was gracious about it. And Betty Anderson did some of the staff work. Ihor Masnyk—who later came into my immediate office and later became Deputy Division Director for Al Rabson—also did some staff work. We spent about six months developing a comprehensive plan. We issued RFPs and reviewed contracts. Ray Bryant brought functionally, but not administratively, his breast virus group into the Task Force.

GC: That was Ray Bryant or Ray Bryan?

NB: Ray Bryant.

GC: With a T, okay.

NB: Paul Carbone ran the Treatment effort, I ran the Diagnostic. Earlier in my career, as the Clinical Director—you're going to think I'm a bad guy, because Ken Endicott turned to me regarding Eli Nadel who was running the Diagnosis Branch—have you heard about that one?

GC: No.

NB: Eli and Ken came apart. Eli is dead and Ken is dead, so I can talk. And he asked me to take over its management with the notion that that would be phased out. So I was phasing out and building up at the same time. And I wasn't very kind to Eli. He was a classmate of mine in medical school, not all that well beloved by most of us. So I became the focal point for Diagnosis, which was an unimportant area of research, as I told you. The important ones were the Drug Therapy and the Viral. And Diagnosis is still comparatively unimportant. Amongst the major things that the Breast Cancer Task Force did was put in a lot of money into Bernie Fisher's National Surgical Adjuvant Breast Project. We had meetings of the whole Task Force. I took them out to San Antonio once, up to Williamsburg once, and I forget where else. And I ran it fairly openly—everybody had to present their work at the annual meetings. They were invited to come. We paid their way. It was my intent to create a forum for discussion and review. Somebody wanted me to invite the Japanese; I invited a number of Japanese, and paid their way. Late in '73 or early '74, Paul Carbone came to me and started to show me some of the data on the adjuvant treatment of breast cancer, with phenylalanine mustard, which we were supporting. The initial data looked extraordinarily good. Have you got a piece of graph paper?

GC: I don't have one in here.

NB: The women were divided into two groups. They already had metastatic breast cancer. One group was started on the drug. The other group was followed without drug treatment. The graph shows the recurrence in both the treated and the untreated women. And there's one curve that looks like this. That's the women who did not get any treatment.

GC: Okay.

NB: Early on in the study, the women who got treatment had a much lower rate of recurrence. It looked like this.

GC: Wow! That is a huge difference!

NB: And that bothered us. It bothered me in particular. Bernie Fisher, as you've probably learned, was comparatively slow to publish. When it looked like this, I discussed it with some friends of mine, mostly outside of the Institute. For example, one of them was Cecil Watson, who was Professor of Medicine at Minnesota. He was a friend of mine on the scientific side because we worked in a comparable area of research. He also came and spent a year at NIH at the Fogarty. Now, here's the dilemma. I wanted to break this data out because what I said, if this is what happened with time, and we waited here for five years or ten years or whatever it is, until the data was absolutely solid, in that interval of time a lot of women would have been denied a good treatment.

GC: You're right.

NB: But I wasn't prepared to tell Bernie, "You're going to publish it." And I conceived the notion of having a big meeting of the Task Force entitled, "A Report to the Profession from the Breast Cancer Task Force." We didn't advertise it widely, we didn't invite the press—and remind me to tell you about Jane Brody—

GC: Jane Brody, okay.

NB: In some way or another, it filled the Masur Auditorium. Do you know that one? The main auditorium at the Clinical Center?

GC: No, I haven't been in there.

NB: And then we had another auditorium up on the fourteenth floor, and we were on the television up there. This was the largest meeting ever held at NIH under those set of circumstances. Mary Lasker came down—

GC: Did she really?

NB: Oh, yes.

GC: Oh, boy.

NB: Her sidekick, Deeda Blair, came. Physicians came from all over. The Program was a broadly-based program. Every one of those five groups within the Task Force reported in the whole day's meeting. But the hidden agenda was to break out the Fisher data.

GC: The Bernie Fisher data.

NB: Bernie now tells me that's what he suspected I wanted to do. Bernie, again, is a good friend. Because in another area we provided the money for Bernie to do his studies of limited mastectomy, which turned out to be very successful. The night before that meeting, Bernie and Carol Redmond and I, and I think Marv Schneiderman, or Sid Cutler met at the Holiday Inn in Bethesda, and we reviewed the data. And everybody said—you know, you could take a ratio of this number to this number and look at it statistically—and they said the data was solid. And Carol Redmond said in her experience—Carol is a biostatistician—this is what we should expect from all the work that had been done. It turned out that this is not what happened. So Bernie was right, and I was wrong, but can you imagine what would have happened if I had been right and we waited two or three years?

GC: Right.

NB: So I had no problem doing that. Rauscher was the Director of the Institute then, and you should recall, Rauscher, Zubrod, and I were Scientific Directors together. And we were very, very close and collegial. Rauscher was not a physician.

GC: He was a Ph.D., right?

NB: And he largely deferred to either Zubrod or me. And then I'll tell you one other thing. Zubrod knew about it. Vince DeVita once said to me, "Nat, when you and Gordon were in the Institute, never was there a more powerful duo."

GC: I can believe it.

NB: Well, I'm being immodest.

GC: That's okay.

NB: So that's the Breast Cancer Task Force—after I left, Al Rabson didn't continue it in the same fashion. The academic community got angry with the use of contracts to support research, and it gradually disappeared. But under its aegis, or the Breast Cancer Detection Demonstration Projects—have you heard about those?

GC: No. We need to talk about those.

NB: In the Cancer Act of 1971—and if you want, we'll talk about Centers sometime—

GC: Okay.

NB: In the Cancer Act of 1971, '70 or '71, there was a provision for cancer control.

GC: Right.

NB: There had previously been a Cancer Control Unit in the NCI, whose original mission—one of them—was to popularize the use of the Pap Test. They also did some work on serum diagnostic tests. Jim Shannon did not like that, and that unit went downtown to one of the other bureaus of the Public Health Service. Shannon's name has come up?

GC: Jim Shannon? Sure.

NB: He once told me when he had cancer—Jim's dead now—he came into my office when I was still a Clinical Director. Jim was a heavy smoker, and he said his larynx cancer wasn't related to smoking.

GC: Oh, really?

NB: Yes. You're getting all these little vignettes.

GC: Yes, I am.

NB: I got to know Jim particularly well towards the end of my career, not early, and after he left NIH. There was a provision for Cancer Control. Scientifically—there was a man named Gershon Cohen, and Bob Egan—Gershon Cohen of Philadelphia, Bob Egan another place, I think in your part of the world, in the Carolinas—going back to some earlier work, which I can't identify, who demonstrated that you could take x-ray images of the breast and find cancer. In the early 1960s, the Cancer Institute, under Mike Shimkin, set up what is known as the HIP Study. Have you heard of that one?

GC: Yes, but I don't know a lot about it. I've just heard that.

NB: I'll tell you.

GC: Okay.

NB: The HIP is an acronym derived from Health Insurance Plan of New York. And that study was a study designed in this way—and again I'll use a diagram.

GC: Okay.

NB: It was a very, very clever design, in many respects. What they did is—the HIP Study was one of the very early HMOs, and they had a large panel, a large number of patients, women, in that panel. And what they did is they took out the records of 60,000 women, and they divided them into two groups. One group were the controls. They never knew they were in the study. They were just followed, and they got their ordinary, regular medical care.

GC: Oh, they had no idea they were part of a study?

NB: No, and so you didn't have to go to that group and ask them to consent to be in a study in which they wouldn't be studied.

GC: Oh, okay, because nothing different was happening.

NB: No. The other group were invited by letter to have one, two, three, four mammograms at annual intervals, and at each time, a physical examination of their breasts. Towards the end of the '60s, the data began to become available. And in this group of women—oh, incidentally, about 20,000 showed up here, and then each time about a thousand less, so that maybe 15,000 or 16,000 showed up for the last study. They then took the breast cancer experience of all 30,000, whether they had one exam or four or none, and they compared this 30,000 [study group] to this 30,000 [control]. And there was a reduction of approximately 30-plus percent in breast cancer mortality in the women who had one or more mammograms or not. And so what did this study do? It tested the whole program—it was really a social question: Would the women come in if it were offered? And if it were offered and they came in, would it reduce mortality? This is the same question that if you were to take that out to society . . .

GC: . . . would the women come?

NB: . . . would the women come?

GC: Right.

NB: So literally, they tested compliance, physical examination, and mammography all together, and it's almost impossible to say what the benefit is of any one alone. Much of it is ascribed to the mammography. And then the American Cancer Society, either at the national level or the New York level, created a Breast Cancer Control Task Force. Now, in those years, we used to meet under Ken Endicott up at Hershey with the senior people of the American Cancer Society, so we got to know each other. At the same time, in a comparable period of time, the senior staff met with Benno Schmidt and his colleagues, the President's Cancer Advisory Board, to discuss cancer control. I well remember the meeting in Building 37, and we could not develop a definition. Benno is a lawyer, I'm not a lawyer, but some of us, including myself, suggested that the way to do it is by the case law method. We would each mention a project, and then the group would say if they thought this was cancer control or not. This was the way the British Law—that's the way the Common Law developed.

GC: Oh, really?

NB: Yes. And then, shortly after that—and I like to draw the diagram—this is a big conference table, on this side are chairs, a couple rows of chairs here, some rows of chairs here, and here's where the National Cancer Advisory Board sat. And shortly after that, about the same time, the American Cancer Society decided that they would divide the country into four regions. They'd create either two or three—I can't remember the number—demonstration projects. They'd put maybe \$100,000 into each, and support them for about two years. I walked past Arthur Holleb (Vice President of the American Cancer Society) at a meeting of the National Cancer Advisory Board, and Arthur turns to me and says, "Nat, we need help." I'd already gotten to Rauscher and convinced Rauscher that for every buck that the American Cancer Society puts in, the Cancer Institute would put in two, and we'd double the number of units and we'd extend it a longer period of time. And under the aegis of the Breast Cancer Task Force Diagnosis Committee, we would write the protocol and we would select the units. At one time in the selection process, we had—do you know what a Site Visit Committee is?

GC: That's another thing I wanted to ask you about. No, I'm not sure I do.

NB: Will you put it on your notes?

GC: Okay.

NB: We had committees going out, small groups, three or four radiologists with some staff, looking at units that wanted to become demonstration projects. At one time we had seven of them on the road at the same time. And we selected them. We developed a model budget. There's a name—has Bailer's name come up to you?

GC: John Bailer?

NB: Yes.

GC: Yes, he just wrote another article, very recently.

NB: A terrible article, in some respects. Bailer was the Acting Associate Director for Cancer Control, which is where the money was coming from. And Bailer and I went up to New York to discuss it with them. Bailer thought he was going to lead that—no way.

GC: Really?

NB: No. I was determined I was going to. And I eventually did, and the Cancer Institute took over the whole process. We actually eventually supported 26 units. We made a decision to collect the data. There was one point where I couldn't get the radiologists to agree on the details of the radiological—which one of the radiological techniques they were going to use, and I regret that still to today. But anyway, it went ahead, and is said to be a very successful program. It really brought mammography to the forefront.

GC: Really?

NB: Now, then, the controversy was, women between 40 and 65 could enter into the study. The HID data, the 30 percent mortality reduction, was for the whole group. Now, there's a statistical technique known as subset analysis. And what they did was they broke out the 40 to 49 year old age group, and attempted to analyze the mortality reduction in that age group. And they didn't have enough cases to come to any conclusion. So they said, "The data does not support a conclusion one way or another." And when they came to me—the American Cancer Society already agreed, and I wasn't going to change it, there was no reason to change it. I said, "The data said this is the group of women that were studied, this is the mortality reduction," and the fact that there weren't enough women in this age group didn't bother me. The other thing is one of my statistical colleagues came to me and said, "Nat, is this going to be a randomized study?" and I said, "No." Because randomization is research. This was a control project.

GC: Oh, okay.

NB: And we were under strict orders not to do research with control money because within the Institute—well, if you want to put another note, I'll tell you about the early days of cancer control.

GC: Yes. That's another good topic. Okay.

NB: So we went and studied, started the women. And Bailer started raising hell. At the same time, we were attempting to reduce the radiation dose. Bailer said, "There's going to be an epidemic of breast cancer." There hasn't been an epidemic of breast cancer. And I'd already left the Institute, so they started modifying the program to not examine women under age 49. And then there have been two contentious meetings. Well, one meeting in '93, which was as biased a meeting as I've ever attended.

GC: Really?

NB: I told the organizers later—they were surprised. And you've undoubtedly read about the Board and the Congress on the recommendations for women under 50?

GC: Yes, that's been a huge controversy lately.

NB: There shouldn't have been, because eventually there was sufficient data from this study to show that there was a mortality reduction in the women under 50, but it didn't occur until about ten or twelve years after the study was started. Did I send you the thing that I wrote on screening for breast cancer in women under 50?

GC: Yes.

NB: So you can read what I said. I've been critical of the way they've handled it. So that's the Breast Cancer Detection Demonstration Project. They succeeded. And now—do you get the *New York Times*?

GC: We get it in the office. I don't read it every day, though.

NB: Within the last week to ten days, presumably on a Wednesday, there has been some polling done, and women between 40 and 49 are going for mammograms.

GC: Really?

NB: Yes. So all the controversy has turned out to be more . . . more heat than light. Basically, the controversy is—there are two groups: they are the epidemiologists, the biostatisticians, and the health policy; on the other side there are the radiologists. But I think there's general agreement about what the data has. One group says, "It's too expensive," and one group says, "We take care of people."

GC: The radiologists say, "We take care of people," right.

NB: And what this group [epidemiologists, etc.] has done is manipulate—that's a bad word—used the data in this instance. You have to understand what screening is. The underlying principle in screening for disease is you go into the community and you search for disease where you think you might find it, and particularly you search for disease that you can treat. And so if you go to set up a screening program, it's essentially the equivalent of the needle in the haystack: you've got to turn over a lot of wheat or a lot of hay before you'll find the needle. So let's take some numbers, just out of curiosity. If you screen 100 women—let's say you screen 100 women, and you find 1 cancer—not a bad end result. So we'll take it for 1,000. You'll find 10 cancers. You reduced the mortality from these 10 down to 7 or 6. So they say, you'll screen 1,000 women to save 4 women's lives. It costs you maybe \$200 to screen a woman. Now, for every cancer you find, you probably do between two and three biopsies. That costs money. These people are bottom-liners.

GC: The epidemiologists and the statisticians.

NB: And the health policy. Bottom-liners. These people [the radiologists] take care of women. And you only have to see a young woman in her 40s with breast cancer who's been diagnosed—Oh! Those were diagnosed by mammography alone, or diagnosed before you can feel it in the breast, a good physician. Ninety percent of those women survived their disease.

GC: That's a strong argument in itself.

NB: But some say it's too expensive. Now, let me show you in writing where I said it was our job to do research, to communicate it to the profession, where appropriate to the public, and the public has to decide what it wants to pay for its medical care. I'm not going to tell the public what they should do or shouldn't do. So that's the Breast Cancer Task Force, the Breast Cancer Detection Demonstration Projects, and my approach to the controversy. It was blown out of proportion—it was made controversial, when it really wasn't. And now that the Swedish data has come in, the probability is that you can get the reduction in mortality in the younger women, the same as you get in the older, over 50. Sure, the yield is much smaller. On the other hand, a woman who is diagnosed at 41 or 42 is going to live much longer than the woman who's diagnosed at 55. You have trouble with these people, or at least I did.

GC: That must have been incredibly frustrating.

NB: Well, that '93 meeting, as I said, was biased, very biased. Okay, let's go with the way you want to go next.

GC: Okay. You said you could tell me about the early days of control at the National Cancer Institute—of Cancer Control.

NB: Carl Baker had become the Director of the National Cancer Institute. There were three Scientific Directors, and one Director of Extramural Affairs—I'm blocking on his name; he went to Texas. Whatever it was, Carl turned to us and asked if we would develop a plan for Cancer Control. It was—Palmer Saunders was the Extramural Director.

GC: Palmer Saunders. He was Grants?

NB: Yes, he was in Grants, independently of the rest of us in those days. We split Cancer Control up. Zubrod was going to develop a control program based on treatment, and he did with some things. I was going to develop one in Diagnosis, and Rauscher in Prevention, and Palmer was going to have something on the Grants side. And we began to develop small things. One of the things I did was set up the program for diagnosis of lung cancer by sputum cytology. I violated a principle. I took some of the money from Cancer Control, with everybody's knowledge, because we set up three Institutions: Mayo, Hopkins, and Memorial, as an Early Lung Cancer Diagnosis Cooperative Group. And we did study cytology and x-ray. We found that screening would not reduce mortality. Recently that's coming into question. That's one thing I did. The other thing I did was to set up a group to study the automation of cytology. The Pap test, a slide is prepared—a lot of slides are prepared—they're reviewed by people who are called screeners, who are not physicians—

GC: Right, they're—

NB: —or pathologists. And whenever they find something they think is positive, they'll show it to the pathologists. And then there will be a random sampling of what they thought was normal. And I thought this was a process that did not lend itself well to quality control. And what I wanted to do was set up a program on automation. And we did start. We didn't get very far. Remind me to tell you about the cell-sorter.

GC: Okay.

NB: That's another thing we got done on the diagnostic side. Where did I go? What tangent did I come off of?

GC: This was just talking about the early days of Cancer Control.

NB: Yes, right. That didn't last very long. Rauscher went out, and I think he got Bailar to develop the Control Program. Bailar didn't last long at it. And Diane Fink came in. When she came in, Zubrod and I, Rauscher—he was after Baker—we were cut out. I got what money I wanted, and I got the project started. So that's the very early days. But that plan, I thought—you know, Carl utilized the resources he had, and I think we could have moved farther if we'd been given the opportunity. You must recognize, as I did, that there were people both within the Institute and outside of the Institute who thought that Zubrod and I and Rauscher had too much power.

GC: Really?

NB: Yes.

GC: Did you perceive it that way?

NB: I thought we were benign. Sure we had power. We had money, we had power. Was it well used? I'm not going to apologize for my use either internally or externally. I made my mistakes, sure.

GC: But it sounds like you used your power wisely.

NB: Yes, we set up a Diagnosis Program. I set up a Diagnostic Radiology Program. They didn't move very far after I left. We did support the development of a Diagnosis Program, a CAT scanner for diagnosis of brain tumors. We developed the only controlled study for the diagnosis using a radiological technique. Bolt, Beranek, and Newman did it. And then Diane Fink took it on, and she lasted awhile, and then I think Peter Greenwald got it.

GC: Peter Greenwald?

NB: And has changed its character, very much so. In large measure, in large measure scientifically. Major prevention is available today. Major preventions available today that have a base in science. Get rid of tobacco, and the Pap test. My division set up the Minnesota study to test fecal occult blood. That's all we have that's very specific. The nutrition thing, you'll have to talk to Peter about. What's the evidence that anything nutritional has anything to do with cancer? I find it very difficult, scientifically. That's just me. And I don't know what research we should be doing. You see, the major problem with Bailar is the data that he has on the change in mortality from breast—from cancer in total, the change in incidence, the change in mortality, I wrote up on the paper that I gave you. It was published in '95; I wrote it in '93 and '94. There's been not much to quarrel about in that data. And what Bailar said in the mid-'80s, when he says, "Today we've got to do prevention research,"—but the thing that Bailar doesn't tell you is what to do.

GC: He just said that something needs to be done, but not—

NB: Have you seen his interview with the *Cancer Letter*?

GC: No, I haven't seen that.

NB: Get it. It's fascinating, because when he said, "Well, we ought to put a third of our money, a half a number," and they asked him, he said, "Oh, well, that's a rubber number." Get it. It's at home on my desk.

GC: Oh, I can get it from the NCI.

NB: Get the one with the Bailar interview. If you're writing a history. And then they've got another screening study set up; prostate, lung, cancer in the ovary. I wanted to set up a comparable one. I couldn't get anybody to agree to it.

GC: You wanted to set up on those same cancers?

NB: A multi-site screening. And my advice was—the advice I got consistently was to test each site individually. So we did a lung, we did a bowel, there was no screening for ovary then. And this was before the PSA came in. So we didn't do one. It would have been a waste. So I really don't know what we can do to prevent cancer today, aside from what I told you. But the tobacco one would reduce the mortality in men by a third. Get rid of tobacco. People don't mention that number when they talk about the tobacco industry. Okay. Where do you want to go to?

GC: PSA. What's PSA? I missed that.

NB: Prostate Specific Antigen.

GC: Okay. You said you wanted to go back to the cell-sorter. What is that story?

NB: In the process of automation, or attempting or thinking about automating cytology, the Chief of Cytology in the Laboratory of Pathology in the Cancer Institute—and I'm blocking on the name; it will surface . . . maybe—

GC: That's fine.

NB: Came to me and said, "There are two fellows at Los Alamos, names Fulwyler and Van Dilla, who have developed an apparatus, or had the plans for an apparatus, that can analyze single cells and sort them." And they came to NIH; we looked at it. We didn't look at the apparatus, there was a plan. I happened to have a friend of mine visiting for the summer who is a biological engineer with a physics background. He looked at the device and said, "It might work." So we put the money into it. We transferred the money to the AEC because they came from Los Alamos. And that's the genesis of the cell sorters. Another thing that I'm pleased to have made that decision. Okay, what else?

GC: Jesse Greenstein?

NB: Jesse Greenstein was Chief of the Laboratory of Biochemistry, and when I came here in '56 or maybe early '57, Zubrod took me around or I went around, courtesy calls for all the lab chiefs.

Jesse Greenstein knew that I was a physician. Jesse Greenstein either didn't know that I was a Berkeley Ph.D.—Jesse Greenstein says to me, "Are you going to be like all those other physicians?" and was very disparaging of the physicians' research. And I said, "Dr. Greenstein, I'm a Berkeley Ph.D. like you." He wrote a very good book on the biochemistry of cancer. It's a good book. He was very prejudiced, biased, about physicians in research.

GC: Didn't like them?

NB: He had within his lab a man named Dean Burk. Has that name surfaced yet?

GC: Dean Burk? I don't think so.

NB: B-u-r-k.

GC: No.

NB: Well, the Cancer Institute had a major advocate for laetrile on its staff.

GC: Oh, and that was Dean Burk? And so what happened with that?

NB: We took the heat, and he eventually retired.

GC: Really? But he advocated it the whole time he was there?

NB: Not the whole time. But he wouldn't do the critical studies, and he advocated—he was advising patients, the patients' families—we caught him on some of this—he was too clever for us, and we didn't attempt to fire him. We just waited our time.

GC: So he was advising patients to go take laetrile?

NB: Yes.

GC: Wow. But, as you said, he eventually left, and so it kind of died down?

NB: Yes, disappeared. Okay, what else have you got?

GC: You said go back to Jane Brody. We were talking about one of the breast cancer meetings, I think.

NB: Do you know who she is?

GC: Jane Brody? Isn't she a newscaster?

NB: A tiny little redhead at that time. After the September 1974 meeting when I didn't set up or arrange for a press conference, and I promised Bernie Fisher if there were any press there, I would shield him from the press, she came to me and berated me, "What right do you have not to give us access to Bernie? What right do you have not to have a press office?" So somebody quickly set up a press office. And I found out who set it up. Do you know Paul Van Nevel?

GC: Yes.

NB: It was Paul Van Nevel. And I gave him hell. I didn't remember it. When I came up here in '92, Paul was very nice and said I was the one "you gave hell to." And he since can't be nice enough to me. He said that was his second day on the job, somebody told him to do something, there was me, somebody else, and that was it.

GC: Well, it's his office that I'm doing these interviews for.

NB: Well, when you see Paul, tell him that "Nat told me"—that's you—"about the 1974 meeting and the press conference" and my attitude.

GC: I'm sure he'll remember.

NB: I did not want the press there. I said, "It's a Report to the Profession." Because that's the way I wanted it to be. I was fundamentally opposed to the Cancer Institute holding press conferences.

GC: Really? On any level, or just at that meeting?

NB: For most everything they've held a press conference about.

GC: Really?

NB: Yes, because it's only given them trouble.

GC: Because it's misinterpreted in the press?

NB: In the *Wall Street Journal*—you can—they called me about Steve Rosenberg once, and towards the end of the article I said, "We appear to have promised much and delivered less." Do you understand?

GC: Yes, I do.

NB: So press conferences, you have something new in a mouse or a fruit fly, or a worm, or in a test tube, and maybe it has potential applications, so you have a press conference, and the applications are here, in the minds of some.

GC: And so you promised this whole possible—this new realm of possibilities, but then it might not work out—

NB: Well, usually most of my colleagues have been very careful. It's in the future. But that gets lost.

GC: What do you mean, "It's in the future."?

NB: They'll say, "Well, we've done this, we can do this, in the experimental animal; we'll go ahead and test it in man." It may take years to test it in man.

GC: But you think the press perceives that—or the way it's reported—

NB: The public perceives it as being more relevant. The public is largely—I think; I can't be sure—the public is largely into, "What can you do for me today? I'm sick. I may be sick tomorrow. My mother has cancer, my brother, my son."

GC: So they grab on to these shreds of hope in a way—

NB: That's right.

GC: —and say, "Well, this is what they're saying they can do." And that gets misinterpreted. I think that's probably true.

NB: Okay. What else have we got on our tick-off list?

GC: Doctor draft, the doctor draft.

NB: The doctor draft came in about 1951 or '2, or '3, somewhere in there, when the military found that they did not have a sufficient number of physicians for the Korean War. And the Congress enacted—or put in, modified, enacted legislation that permitted the drafting of physicians. We were the only people—the only men; they didn't draft women in those days, or ever—we were the only group that were drafted by profession.

GC: Doctors.

NB: Yes. The Public Health Service has a uniformed component.

GC: The Commissioned Corps, right?

NB: The Commissioned Corps. And it used to have hospitals. It is the oldest of our nation's hospital system. It goes back many years. Because the Public Health Service provided the health care for the Merchant Marine, going back to the early 1800s. The Public Health Service also provided the medical care to the Coast Guard.

GC: I didn't realize that.

NB: And so in some way or another, physicians could be drafted into the Public Health Service for the Coast Guard, and somebody got them to agree to letting those draftees have one avenue that they could volunteer for, and that was NIH.

GC: So that was how you got a lot of your people.

NB: And so we were able—do you know the expression of Halberstam's, "The best and the brightest"?

GC: Yes. I've heard that phrase before.

NB: So we used to see the most able senior medical students in the country. They came from a limited number of institutions. We interviewed them, as I said, in that matching program. And that program lasted only for the duration of the draft.

GC: And the draft ended in the '70s, right?

NB: Yes. Do you know who one of the beneficiaries of that is?

GC: Who's that?

NB: Varmus.

GC: Really?

NB: Yes.

GC: No, I didn't know that. I think Dr. Freireich came in that way, too.

NB: I don't know whether he was drafted or not, I forget. He was already at the Institute when I got there. He probably did.

GC: I think he volunteered for the NIH. I believe that's how it worked.

NB: Yes, well, if you volunteered, you didn't get drafted.

GC: Right.

NB: Nobody was actually drafted, hardly, because the end result of a draft was you became a private in the Army.

GC: Even as a doctor?

NB: If you didn't volunteer to join.

GC: Oh, I didn't realize that.

NB: So everybody volunteered, but they volunteered with a gun in their back.

GC: So it was kind of a forced volunteer, because you didn't have a real other choice.

NB: So we saw—I forget how many we interviewed each year, but they were absolutely superb people.

GC: So it really benefitted the NIH, and your service in particular, it sounds like.

NB: It benefitted all of NIH. I'm trying to think—the current Director of NIH, the Director of the Aging Institute, the Chief of that AIDS Program, Don Fredrickson—

GC: Oh, he was too?

NB: Yes.

GC: I didn't realize that.

NB: I'm pretty sure he came under that. And there's a whole group in the—a fairly substantial number of the leadership of American medicine had gone through NIH under that program. And one of the things—we weren't very good about history. I don't think anybody outside the Cancer Institute—the Cancer Institute has the record of everybody who came, because I put it together. But I don't think any of the other Institutes have a list of all that they had taken—I just don't know.

When the doctor draft went off, physicians didn't apply anymore, and they [the NIH] had to seek alternative ways of bringing in young people. A lot have come from Europe, but not as physicians. It hasn't been easy to get the same level of young physicians.

GC: Really?

NB: The same level of demonstrated academic ability. Besides, it was very convenient and made it very easy to recruit.

GC: Sure.

NB: We held all the power. The administration of it was easy. We interviewed—sure, it took us time to interview. There was an office that we sent our nominations to, they matched it, then we'd call them up and offer them a job. Broder came that way.

GC: Oh, he did?

NB: Yes. Broder actually came into the Metabolism Service as I was phasing out.

GC: That's right. I'm talking to him on Thursday.

NB: I know. You said you're going to Miami.

GC: Yes. Well, even without the doctor draft, wasn't there—well, let me ask that in a different way.

NB: I think NIH—I said it in my paper. The doctor draft, more than anything else, made the NIH.

GC: Really?

NB: Yes. We had the money—NIH had the money, had the building; didn't have the people. And it never would have gotten the people . . . easily.

GC: Without the doctor draft.

NB: Without that. And anybody that tells you otherwise is not being honest with themselves. Because we clearly demonstrated in '72 and '73 and '74, whenever it was, and the first year after the doctor draft went off, the number of applicants dropped by at least a half, and subsequently went down to two-thirds, and then it gradually disappeared. And they set up alternatives.

GC: To the draft—I mean, to—

NB: Not to the draft; to finding people.

GC: Steve Rosenberg came in through the doctor draft?

NB: Yes, sure.

GC: I didn't realize that either. So you really pulled in a lot of people. Almost everyone—

NB: And I guess it was Zubrod's and my job during the decade I was the Clinical Director to be sure there was a climate that encouraged them to do research, encouraged men who would not have thought about it otherwise to get into research. Varmus came wanting to be an endocrinologist with Ira Pastan. When Ira came over to the Cancer Institute, he [Varmus] came over. Al Rabson tells me, "Nat, why didn't you, when he came over, put him on the Cancer Institute payroll?" The one thing I guarded zealously was the numbers of people on the payroll, and if I could get somebody for free, I did it!

GC: Sure!

NB: Well, you'd have to go through the Institute list. I forget the other—Phil Gordon from the Arthritis Institute was probably in the draft. Well, whatever it is, that's unimportant. But I think that's the single most thing. And I said if I could be a dictator today, I would reinstitute the doctor draft for that purpose.

GC: Oh, really? Just to bring in those—

NB: Just so that American medicine twenty years from now will have had a cadre of very able people who got their introduction to the research world through NIH. As you probably are beginning to hear, it is very difficult to do research in medical schools, and it's very difficult to do clinical research today outside of NIH.

GC: Really?

NB: Very difficult.

GC: What is it about NIH that makes it possible?

NB: Everybody has a salary. You don't have to go out and get a grant.

GC: So that whole level of pressure is taken off.

NB: There's no pressure. The only pressure at NIH is to produce new information . . . on the research side. The other pressure on the clinical side is to take care of patients and do it well. That's the only pressure.

Sure, you get reviewed, but not in the same way. It's a retrospective review; not a prospective. Grants are prospective review. The Boards of Scientific Counselors are largely retrospective review.

GC: Just to make sure that you are working and you're making—

NB: What I used to do with my Board, which met twice a year, I tried to alternate between a very good unit which was entertaining and educational and one that was ordinary and one that was giving me a problem. I once took a problem unit to them, and they said, "Nat, it's better than you think it is."

Okay. What else have we got on our tangent list? I'm not pushing—I don't mean to push you. I've got nothing else to do.

GC: No, no, this is fine. You said you wanted to go back and talk about the Centers.

NB: Oh, yes. There are somewhat different views about how the Centers Program got organized. But before the National Cancer Act, there were small grants to medical schools in which they appointed somebody to be a coordinator of science or cancer in the medical school. Some people, I think, credit this with the origin of the Cancer Centers Program. But more importantly, the National Cancer Act of 1971 called for the creation of fifteen additional Cancer Centers, and the models were supposed to be M.D. Anderson, Sloan-Kettering, and Roswell Park. The legislation also said that they could get grants up to \$5 million. And that's never been done, in the sense of creating a new Roswell Park, or a new M.D. Anderson, or a new Sloan-Kettering. And instead we began to make Center grants to universities. Before that, while Ken Endicott was still a Director—and if you ever get to talk to Jesse Steinfeld, he'll tell you more about it—Ken discovered or found out that there were at Sloan-Kettering or Memorial—whatever it is, the same institution—there were fifty grants. And he conceived a notion of combining these all into one, setting up a review committee to review that institution, all the grants, and I think he got agreement from the leadership there that this would constitute up to but no more than 50 percent of their research money, so that the NCI wasn't completely supporting them, and they had to go out elsewhere and get some support, in which case the NCI would have less of a problem in reviewing that institution, but more particularly have fewer grants to review because they'd have the one major one. That lasted for a while, the academic community complained in a sense. They said, "It's too big to review." One of the things about the academic community is they're—the strong word is "obsessed" with review.

GC: Really?

NB: I spent 19 years at NIH. We reviewed ourselves internally rather vigorously, but we didn't have study sections, we didn't have grants, we had a Board of Scientific Counselors, each scientist wrote an annual report. The internal review—the Scientific Directors met as a group under the Deputy, and each year every Scientific Director presented at that review every doctor or person in that Institute and singled out those that he was going to recommend for promotion, and may have also highlighted some who weren't doing very well. Before anybody could get promoted within an Institute, it was reviewed by the Scientific Directors of all of NIH. And I can tell you, my colleagues were not easy.

GC: Really.

NB: No. Yes, the promotion rate, we generally accepted it, but everybody knew how high the hurdles were. So you didn't bring what I called stinkers. You just didn't bring anything that you didn't think you could get through, because every once in a while if you'd bring someone that wasn't, you'd quickly to hear about it and learn.

GC: So it was a big deal to get promoted. It wasn't just an automatic . . .

NB: Oh, no. Then it became apparent that civil servants were tenured after a year of civil service. So we changed that and introduced the Staff Fellowship Program so that we could have a longer period of observation before anybody got a permanent appointment.

GC: So you didn't have to follow the civil service rules.

NB: For ordinary civil service.

GC: Right.

NB: These are things that the university community doesn't like to acknowledge readily, that we were rigorous. And I can only tell you this: one time I took them to my Board—a group that wasn't doing well and some individuals that weren't doing well, and one of my Harvard friends said, "Nat, we have our fair share of deadwood. So don't ask anybody to come in at age 25 and be a productive investigator at 65; it doesn't occur very often." Okay, what else? I'm not hurrying you.

GC: No, you're not. We're doing fine. This is fine. I wanted to ask you about working with the Directors of the National Cancer Institute. You worked with Endicott, and Baker, and Rauscher.

NB: Heller.

GC: Oh, you worked with Heller, too?

NB: Yes. Heller was Director when I came in—Endicott, Baker, and Rauscher. What would you like to know?

GC: Well, I'd like to know how much contact you had with them on a daily or weekly or monthly basis. Were you making your decisions independently and kind of reporting to them? Did you talk to them at all? What was the relationship, really, the working relationship? And you can take it individually, if you want, or as a whole.

NB: There are two aspects of how to answer that. Each of the Institutes—and it's also reflected at NIH—but each of the Institutes has an Intramural Program and an Extramural Program. The Director is responsible for both. At the NIH level, there's a Deputy Director for Intramural Research, at least in my time. We used to meet on the first and third Wednesdays of the month at nine o'clock often going until one. As I said, all Intramural promotions—not Extramural promotions—went through that group. And we reviewed—we called it a Program Review.

And we'd occasionally listen to some science. We once went down to the Research Triangle [in North Carolina] to hear some science when Paul Kotin set that up originally. Either that or the Cancer Institute did it. I know I went there. As I told you, my position as the Clinical Director was within the Office of the Director of the Institute. So I had literally statutory authority to go to him directly. There was no way—if I went to the Director, I wasn't end-running anybody as the Clinical Director.

GC: That was absolutely—

NB: Now, that's one thing. Then the Clinical Directors used to meet as a group under the chairmanship of Jack Masur, but Bo Mider used to come regularly. So now you had two groups, the Scientific Directors and the Clinical Directors, meeting together under the aegis of a Deputy Director for NIH. Masur actually had the title of Associate Director, but essentially a Deputy Director. And they were the ones that in large measure controlled much of the Intramural Research. And there was a bit of resentment amongst the Institute Directors over the role of the Deputy Directors, particularly Mider, in Intramural Research, when he was the Director because we used to go to him, we sat with him, we met with him regularly, we had access. Carl Baker, I think more incorrectly than correctly, thought I was end-running him.

GC: Oh, really? By going to the—

NB: When I went to the Deputy Director—because he was the end—when I wanted to hire Sherm Weissman and get him a salary, I had to go see Bob Berliner. It didn't matter what anybody else in the Institute said. Now, Bob and I were friendly. We had both been Scientific Directors together before I became the Chief Scientific Director.

GC: So this just made sense for you to go to him.

NB: Yes.

GC: But Baker still maybe felt that you were . . .

NB: Baker thought I was end-running. And in a sense I was. But it had to be a matter of trust. Baker wanted to control things more than anyone—Intramural—than any one of the other Directors. Ken Endicott—when Ken Endicott became the Director—did Zubrod talk to you about the Scientific Directorate?

GC: A little bit, but—

NB: Well, it eventually transformed itself into the Executive Committee. And each one of the Division Directors, the four of us, would meet with some staff. Ken Endicott used to come frequently and sit through the meeting. Carl came. Rauscher—I think I left fairly soon after—well, Rauscher and I—as I told you, Rauscher and I and Zubrod were Scientific Directors together. Mider recognized only one Scientific Director in the Institute. For a long time the others were not invited to the Scientific Directorate meeting, but I was. But we were very—the three of us worked together closely, we understood each other, and that was it, in a sense. Palmer Saunders, on the Grant side, was very independent. He didn't want us to know what he was doing. I wish I didn't know some of the things he was doing. I've got to come back to Centers with Palmer Saunders.

GC: Okay.

NB: So I think that's how—Rod Heller wasn't a Director very long, but I saw him occasionally. I was comparatively way down the totem pole then. I became Chief of General Medicine, I think under—I can't be sure whether it was Heller or Endicott; probably Endicott. I certainly became the Clinical Director under Endicott. And I became a Scientific Director under Endicott. Endicott was very trusting. And we could speak to him. Endicott looked for help, he looked for advice, looked to consult with his staff. And, as I think I told you, for a while the Division Directors were meeting before the Scientific Directorate without some of the other staff—Cal Baldwin and others who sat with us but may not have had a vote—and we were called the "Dawn Patrol," largely because some of the more contentious things we discussed amongst ourselves before it came to the Scientific Directorate. All right. The Centers—they went and developed some guidelines. They changed them over the years. They expanded the number. They created a committee to review them. I went to Northwestern University; they'd already gotten their Center grant, but they didn't have a Director, and they offered me that job. It was a much harder job in some respects than my job here at NIH, because here I had the resources, at the university I didn't. So you make do with what you can. They're now changing some of the guidelines that the Centers have to adhere to in the Review Process. Parenthetically, I'm going back to the Cancer Center in Miami as a Senior Advisor to the Director.

GC: Oh, really?

NB: Yes. Which I want to do.

GC: When does that start?

NB: Now. They're going to announce it. It's an informal sort of thing. But the Director says he looks forward to what I can do.

GC: I'm sure.

NB: Okay. So that's the Centers Program.

GC: Was there something you wanted to say about Palmer Saunders?

NB: Palmer didn't like me very much.

GC: Really?

NB: I don't know why. He got the notion—turn that machine off for a minute. There's no sense in recording this—I can be ungentle at times. What else would you like to do?

GC: Well, I'd like to know what you think your greatest contribution was during the time you were at NCI. Another way I sometimes ask the question is, what did you enjoy the most?

NB: Well, they're two different, they're two very different.

GC: Okay.

NB: What I truly enjoyed the most was the Metabolism Service. I kept my position there from, as I said, from '56 to maybe '71 or '72, somewhere along in that era. I kept my lab there until I gave it up. And even after that, I kept my affiliation there. That was to me the science that I enjoyed the most.

GC: You enjoyed the research in particular, or building the—

NB: Well, we were a small group, we were very collegial. We reviewed each other's work. When the research meetings came, we would rehearse amongst ourselves. And somebody would show a slide and [somebody else would] say, "I don't know what that says. I don't understand what you mean." And to me this was the collegial—I've use that word all too often; I rarely use it—it's the cohesiveness and the collegiality that I'd like to see in the research world.

GC: Just the idea that people would say whatever they needed to say and ask questions?

NB: Yes. And when I came up in '92, Tom Waldmann asked me to participate in that review again. And I did. And I was accepted. Of course, for many of them, I'd been their Chief.

GC: Right.

NB: Some of them. Now, contributions, I don't know that any—I'll put them down in some sort of order, and then you can pick.

GC: Okay.

NB: Shortly after I came to work, I went to Zubrod when I was the Clinical Director—when I was Chief of General Medicine and he was the Clinical Director—and I said, "I'd like to expand the number of clinical associates we're taking on." And we went to Mider, and he said, no. The day he left, figuratively, we expanded it.

GC: Oh, really?

NB: Increased it. We took on more than any other Institute. I think between Zubrod and I, each in his own way, a major contribution was the development of the Clinical Research Program of the National Cancer Institute, either through our guidance or increasing the number of clinical associates or our appointments of Lab Chiefs or Branch Chiefs. And it turns out, as I think I pointed out to you, the enormous contribution, I think, that the Clinical Associate Program has made to all of NIH and in the cancer world to the leadership of the cancer world today. So that's one. The other contribution, intramurally—another contribution, intramurally—is what I think I intimated to you. I took the general laboratories and clinics, and as Bo Mider said, "Nobody ever made more changes," whether it was in biochemistry, in molecular biology, in pathophysiology, or mathematical biology, at the same time phasing out physiology, endocrinology moved to another Institute, and the appointment of—well, one of the other contributions, the hidden one is Steve Katz, who came into Dermatology, is now running an Institute. So remaking that group. And Bob Berliner, who was a great scientist—has that name—am I the only one who has mentioned him, or is that—

GC: No, I've heard his name before. I don't know a lot about him, though.

NB: When Bob Goldberger came over to the Cancer Institute as Chief of Biochemistry, Bob Goldberger said to me—again, this is being immodest—I was the last bastion of science in the Cancer Institute.

GC: Oh, really? He said you were the last bastion of science?

NB: My division, who were represented by me. Because the others have not done as well scientifically—let's face it—with some exceptions. They were mission-oriented. So that was—to leave—when I left, and Al Rabson took over, he had a really superb unit, the Division of Cancer Biology and Diagnosis. Now, let me think, what else. That's to some extent on the Intramural side—although John Doppman, who was Chief of Radiology when I was up here in '92, seemed to imply that the best thing I did was get Steve Rosenberg here. On the Extramural side, I'm particularly pleased with what the Breast Cancer Task Force was able to do and accomplish, and how we ran, until I left, the Breast Cancer Detection Demonstration Projects. So, you know, you can't—it would be unfair—it would be wrong of me, not unfair—you can ask the question, it was a good question—but I played different roles. And in the different roles, these are the things that I think have had the most lasting effect.

GC: And they have. They have had a lasting effect. So, when you left the Cancer Institute, why did you choose to leave?

NB: From 1969 until 1974, those of us who were at the top of the Civil Service didn't get a pay raise. And Sheldon Wolff said—who later went to Tufts—that that was a time when there was a 41 percent inflation in those years.

GC: Oh, really?

NB: I missed Zubrod when he left. As I said, we worked together well. Vince DeVita came on as Zubrod's successor. I played some role in that. I didn't think that I was going to be able to work as well with DeVita as I did with Zubrod. He may not say so. And then out of the blue came the job offer from Northwestern, and my wife liked the idea. Vince DeVita once told me, "Nat, you managed to move or change at exactly the right time."

GC: Really?

NB: In retrospect, my move to Northwestern University satisfied my ambitions academically. Financially it was very, very good. It gave me the base to move to Miami for my last move, which was, again, due to Zubrod—I followed Zubrod all along the line. If you want to say we're a pair, you can. He was very good to me.

GC: Zubrod?

NB: Yes, I succeeded him as Chief of General Medicine, I succeeded him as a Clinical Director, I became a Scientific Director with him, I was Chairman of his External Advisory Board, when I was finishing my term at Northwestern University he brought me down and created a good job for me that was very rewarding financially. In my early 60s, my wife asked me, "Where do you want to live when you retire?" I said, "Where it's warm all year round." She said, "I'm not going to Florida." We did go to Florida. And at that time I became his Deputy again. When he left, I ran the Sylvester Cancer Center for a year or so until they got a permanent Director. But he's been—there hasn't been a time since I left the Navy that he hasn't helped me achieve.

GC: He seems like that kind of person, too.

NB: He's very nice. He won't give you some of those cutting comments that I gave you. He's much too gentle.

GC: You came back for a year. Is that right?

NB: Yes.

GC: And you worked with Dr. Rabson?

NB: Yes.

GC: What was—you were a Guest Worker? Is that the term?

NB: Yes. I was supposed to go back on the staff. They had a personnel freeze, and I got frozen out, but I was a Guest Worker. Al set me up, got the Library to set me up in the stacks of the National Library of Medicine. I did some writing, I did some reviewing, I made some rounds, I saw my friends, I gave them a little advice—not much.

GC: Did you enjoy it?

NB: Enormously. If—again, I said to you earlier, that if I could have gone back to Berkeley, I would.

GC: No, I was just about to wrap up. If you just want to finish talking about this for a second?

NB: No, I don't have to. You're free to latch onto me for as long as you want.

GC: Oh, I know. I think we should stop pretty soon, though, just because our voices are going to get tired.

NB: Well, Rabson wanted me to do something on diagnosis. I did, but didn't finish it. I wrote that *Conquest of Cancer* history. I wrote the Metabolism [Branch] history when I was here. I did sit with the Director at scientific meetings. As I told you earlier, I would have enjoyed enormously, in the mid-'60s, going back to Berkeley. I would enjoy it enormously if I were living in Bethesda. I live very well in Miami, and I'm not going to move. NIH made me—I think I told you that I had an NCI post-doctoral fellowship, I had a National Heart Institute Special, and I had a Fogarty International Senior Fellowship. I'm a product of the NIH. I was the Principal Investigator on Core Grants, the ECOG Grant, and construction grants.

GC: Wow.

NB: I think the NIH is a demonstration that the federal government can do things very well.

GC: I think so, too.

NB: It is the world's Mecca for biomedical research. And if you want to say I came early, comparatively early in its post-war history and played a role, then maybe that's it.

GC: Would you like to stop at this point?

NB: I'll go on as long as you want.

GC: Okay. Well, the only—I just wanted to ask you who else you thought I should talk to.

NB: You know, I haven't asked you to tell me exactly what you're trying to do.

GC: Oh, okay.

NB: Except get an oral history or write a history.

GC: Right. At this point, we're collecting oral histories on tape. We want to make sure that—it's kind of a push to get the National Cancer Institute's history preserved, and this is one of our—

NB: Yes, we were very deficient.

GC: —first big projects is to just get people talking on tape, like you're doing right now, about all these little details that no one else can fill in, because you were there and you know them.

NB: Mider, when he left the Deputy Directorship and went to the Library, said he wanted to do a history, but said he couldn't find the data. Vicky Harden is attempting. I gave her some material, but not much.

GC: Well, that's what this is all about, is collect what we can right now, and from here, we'll just have to see where we go. But right now it's just really an attempt to pull this all together and get some information about what happened, because the written record only goes so far, as you know.

NB: From the historical point of view, one of the things we did in my office is we published an informational bulletin in the mid-'70s to about '80 which listed all this past clinical research and staff associates, and that should be available to you. If it isn't, I'll send you mine.

GC: Okay.

NB: Or I'll send it out if you promise to copy it for me.

GC: Okay. I'll see if I can find that up here.

NB: The other thing you might want to do on the historical side—and I don't know how much this is going to help you—is get the Appropriations Committee testimony of the Directors of the NCI.

GC: I have a little bit of that.

NB: Well, you know, there will be both House and Senate, and there will be Authorization Committee and Appropriations Committee.

GC: Authorization and Appropriations, okay. That's a good idea. I do have just a little bit of that testimony.

NB: You ought to speak to David Rall.

GC: Okay, he's on my list. And he's still in this area. Is that right?

NB: Oh, he went from here down to Research Triangle. When he finished as Director of the National Institute of Environmental Health Sciences, he came back here.

GC: Do you know Harold Stewart?

NB: Yes. He doesn't like me.

GC: Oh, okay.

NB: From his standpoint, for good reason.

GC: Oh. He's someone I wanted to interview, and he won't talk to me right now. He hasn't agreed to an interview.

NB: He's in his 90s; I don't know how lucid he is these days. When I became the Scientific Director, I began taking a look at what we had. We had a Laboratory of Pathology, which was part of the Cancer Institute. In the Clinical Center, there was a Pathological Anatomy Branch. Stewart ran both of them. The personnel were paid for by the Cancer Institute, and Red Stewart used to try to play one of his roles off against another, and then I found that he had about a quarter or a fifth of all the resources.

GC: Of the whole Institute?

NB: Of my Division.

GC: Oh.

NB: Then I found—I don't know whether Red was Acting then—no, Thomas was—that there were 32 people in one building that were doing nothing but cutting animal tissues for pathology. And what they forgot was that I spent a year in pathology and I know how to cut tissue. And they forgot that there were people who would do it for a price in the community. So I gradually phased that out because I didn't want the space; I wanted the positions. The most valuable thing at NIH was positions. And I phased that out. Do you want to hear another little vignette?

GC: Yes.

NB: My children volunteered for the summer. My daughter quickly learned when anybody asked her name—you know, when I was—she was assigned to the Clinical Center—what her name was: "Debbie"—she would never say Berlin.

GC: Oh, really?

NB: My son worked for the Surgical Group. And one day he got some tissue and he was told to take it over—animal tissue—to take it to that laboratory that cut animal tissue, and he was supposed to ask the question when it would be ready. So he did. And he got a stinging rebuke. "That fellow, Doctor Berlin, he's cutting us back, I don't know when it will be ready," et cetera, et cetera, et cetera. My son never told me who told him that. And that man didn't know he was my son.

GC: Oh, really? Probably a good thing.

NB: Well, for him. I never knew who it was.

GC: Anyone else you can think of that I should talk to in particular?

NB: If you're in Miami—No, Ketcham's up north; you ought to talk to Ketcham.

GC: I think he summers in New York. Is that right?

NB: Up at Lake Erie.

GC: Yes, he's someone I want to get.

NB: I'd get Palmer Saunders. Palmer must be 80 these days.

GC: Okay.

NB: He'll give you a different view of Grants. Rauscher's no longer living. He's dead. Baker?

GC: Yes, I've spoken to him once, and we're talking again next week.

NB: Did I give you this?

GC: Actually, I do have a copy of that from Victoria Harden.

NB: Mider is no longer alive. I presume you're going to talk to DeVita.

GC: I talked to him.

NB: You have?

GC: Yes, I have. I went up to see him.

NB: Did you get what you wanted?

GC: Yes. I think I need to talk to him again. We only talked for a little over an hour. He was pretty busy.

NB: He was probably frank.

GC: He was. He was very frank.

NB: Paul Carbone would be very helpful.

GC: Okay. He's on my list.

NB: And one—I'll suggest you go up to Pittsburgh and see Bernie Fisher.

GC: Oh, really?

NB: He may be very candid with you. He's very bitter and he's very angry.

GC: Would he talk to me, being from the National Cancer Institute?

NB: Tell him what your role is.

GC: Okay.

NB: You're only doing it for them. You're not paid by them directly.

GC: No, they are paying me. They're paying our company to do it.

NB: But not directly. You're not on their payroll.

GC: No, I'm not on their payroll, that's correct.

NB: You're an employee of a contractor.

GC: And I'm also an historian, which means I'm neutral.

NB: There are revisionist historians.

GC: Yes, there are. I am not one. You're right.

NB: One of the men who's still in the Institute came in July '56, is Waldmann.

GC: That's right. He's still there.

NB: He'd give an extraordinarily good picture.

GC: Okay.

NB: Have you thought of talking to any of the other Scientific Directors of the time? That would be Berliner, or Ed Rall.

GC: Well, Ed Rall was in a different Institute though, right?

NB: Other sides . . .

GC: Other Institutes?

NB: Of their perception of the Cancer Institute.

GC: I had not thought about that, but that's a good idea.

NB: You're going to find there a very different view . . . if they speak to you as candidly as they used to speak to me. I didn't tell you that vignette about Berliner and Goldberger for nothing [see pp. 40-41]. Lou Carrese is not alive. Cal Baldwin, who was our Executive Officer for a long time, would be very helpful. But you know there's a large number of people who played major roles.

GC: Yes, it's a big place.

NB: You might want to talk to Claude Klee or Maxine Singer about their perception of biochemistry and how the Laboratory of Biochemistry evolved . . . and I'd add Steve Rosenberg. And then I'd call it quits. I don't know how much of what I've told you, you've heard from others.

GC: Actually, you told me a lot of new stuff. You've expanded on a lot of it definitely. Well, why don't we end the tape here.

NB: Okay, fine.